ON THE METHODS OF THEORETICAL PHYSICS.

[A lecture of Boltzmann's on "The Recent Development of Method in Theoretical Physics" was translated in *The Monist* for January, 1901 (Vol. XI, pp. 226-257). But this earlier lecture should be read in connection with it. An exhibition of models, apparatus, and instruments used for the purpose of mathematics and mathematical physics was planned by the German society of mathematicians (*Deutsche Mathematiker-Vereinigung*) for the meeting at Nuremburg in 1892. Such an exhibition had been held—on a larger scale—in London in 1876, and since then the question of models had increased very greatly in practical, theoretical and pedagogical importance. At the last moment the planned exhibition was postponed till September, 1893, when it was held at Munich. Among the eight essays written for and published in the catalogue of this exhibition,[1] issued in 1892, was one by Boltzmann "Ueber die Methoden der theoretischen Physik"[2] which is here translated. An English version was communicated by the Physical Society to the *Philosophical Magazine*,[3] and it is this translation which has served as a basis for the present one. For permission to make use of it I am indebted to the publishers of the *Philosophical Magazine*. The omissions and errors in the translation have been rectified with the help of the original German. I have also verified and completed the references. The additions made in the translation in the *Philosophical Magazine* are given in the Supplementary Note following the essay itself.—P. E. B. JOURDAIN.]

CALLED upon by the editors of the *Katalog* to deal with this subject, I soon became aware that little that is new could be said, so much and such sterling matter having in recent times been written about it. An almost exaggerated criticism of the methods of scientific investigation is indeed a characteristic of the present day; an intensified "critique of pure reason" we might say, if this expression were not perhaps somewhat too presumptuous. It is not my object again to criticize this criticism. I will only offer a few guiding remarks for those who, without being

[1] *Katalog mathematischer und mathematisch-physikalischer Modelle, Apparate und Instrumente,* edited by Walther Dyck, Munich, 1892; Nachtrag, Munich, 1893. This essay was reprinted in Boltzmann's *Populäre Schriften,* Leipsic, 1905, pp. 1-10.

[2] *Katalog,* 1892, pp. 89-98.

[3] *Phil. Mag.,* 5th series, Vol. XXXVI, 1893, pp. 37-44.

specially occupied with these questions, nevertheless take an interest in them.

In mathematics and geometry, the necessity for economizing labor undoubtedly led at first from purely analytical to constructive methods, and to their illustration by models. Even if this necessity appears to be a purely practical and obvious one, we here find ourselves on ground on which a whole class of modern methodological speculations has grown up, which have been expressed by Mach in the most precise and ingenious manner. He, indeed, directly maintains that the sole object of science is economy of labor.

Seeing that in business affairs the greatest economy is desirable, it might almost with equal justice be maintained that economy is simply the object of the salesroom and of money in general, and in a certain sense this would be true. Yet when we investigate the distances, the motions, the magnitudes, the physical and chemical nature of the fixed stars, when microscopes are invented and we thereby discover the origins of disease, we shall not be very willing to describe this as mere economy.

But what we denote as an object and what are the means for attaining that object are after all matters of definition. What we regard as existing—whether we so regard bodies, or their kinetic energy, or, in general, their properties—depends in fact on our own definition of existence, so that we may perhaps at last define away even our own existence.

But let this pass. The necessity exists for the most complete utilization of our different powers of conception; and since it is by aid of the eye that the greatest mass of facts can be surveyed[1] simultaneously, it becomes desirable to make the results of our calculation perceptible,

[1] We say characteristically enough "*Uebersehen.*" [In English an "*Uebersicht*" would be translated by some such word as "survey." Literally it is "oversight."]

and that not merely by the imagination, but visible to the eye and at the same time palpable to the touch by means of gypsum and cardboard.

How little was done in this direction in my student days! Mathematical instruments were then almost wholly unknown, and physical experiments were often made in such a manner that they could only be seen by the lecturer himself. And as, further, owing to shortness of sight, I was unable to see writing on the blackboard, my imagination was constantly kept on the stretch. I had almost said luckily for me, but this statement would be in opposition to the object of the present catalogue, which can only be to praise the infinite equipment of models in the mathematics of the present day; and it would, moreover, be quite incorrect. For even if my powers of imagination had gained, it could only have been at the expense of the range of my acquired knowledge. At that time the theory of surfaces of the second order was still the summit of geometrical knowledge, and an egg, a napkin ring, or a saddle was sufficient for illustration. What a host of shapes, singularities, and of forms growing organically out of each other, must not the geometrician of the present day impress on his memory! And how greatly is he not helped by plaster casts, models with fixed and movable strings, links, and joints of all kinds!

Not only so, but more and more way is being made by those machines which serve not for mere illustration, but save the trouble of making actual calculations, from the ordinary four rules of arithmetic to the most complicated integrations.

As a matter of course both kinds of apparatus are most extensively used by physicists, who are continually accustomed to the manipulation of all kinds of instruments. All conceivable mechanical models, optical wave-surfaces, thermodynamical surfaces in gypsum, wave-machines of

all kinds, apparatus for illustrating the laws of the refraction of light and other laws of nature, are examples of models of the first kind. In the construction of apparatus of the second kind some have even gone so far as to attempt the evaluation of the integrals of differential equations which hold equally for a phenomenon difficult to observe, like the friction of gases, and another which allows of easy measurement, like the distribution of an electric current in a conductor of suitable shape, and then, by observation of the latter, to utilize these values for the determination of the constants of friction. We may also remember the graphical evaluation of the series and integrals occurring in the theory of tides, in electrodynamics and so on, by Lord Kelvin, who in his *Lectures on Molecular Dynamics* even suggests the establishment of a mathematical institution for such calculations.

In theoretical physics, other models are gradually coming into use which I am inclined to class as a third species, for they owe their origin to a peculiar method which is being applied more and more in this branch of science. I believe that this is due rather to practical physical needs than to speculations in the theory of knowledge. The method has, nevertheless, an eminently philosophical stamp, and we must accordingly enter afresh the field of the theory of knowledge.

At the time of the French Revolution and afterwards the great mathematicians of Paris had built up a sharply defined method of theoretical physics on the foundation laid by Galileo and Newton. Mechanical assumptions were made by means of which a group of natural phenomena could be explained, and these principles had attained a kind of geometrical evidence. Men were conscious that the assumptions could not be described as correct with apodeictic certainty, yet up to a certain point it was held to be probable that they were in exact conformity with

fact, and accordingly they were called hypotheses. Thus, matter, the luminiferous ether for explaining the phenomena of light, and the two electrical fluids were imagined as sums of mathematical points. Between each pair of such points a force was imagined to act, having its direction in the line joining the two points, and whose intensity was a function, still to be determined, of their distance (Boscovich). A mind knowing all the initial positions and initial velocities of all these material particles, as well as all the forces, and which could integrate all the differential equations arising out of them, would be able to calculate beforehand the whole course of the universe just as the astronomer can predict a solar eclipse (Laplace). There was no hesitation in declaring these forces, which were accepted as originally given and not further explainable, to be the causes of the phenomena, and the calculation of them from the differential equations to be their explanation.

To this was afterwards added the hypothesis that, even in bodies at rest, these particles are themselves in a state of motion, which gives rise to thermal phenomena, and the nature of these particles is very accurately defined especially in the case of gases (Clausius). The theory of gases led to surprising prognoses; thus, for instance, that the coefficient of friction is independent of the pressure, certain relations between friction, diffusion, and conductivity for heat, and so on (Maxwell).

The aggregate of these methods was so productive of results that to explain natural phenomena was defined as the aim of natural science; and what were formerly called the descriptive natural sciences triumphed when Darwin's hypothesis made it possible, not only to describe the various living forms and phenomena, but also to explain them. Strangely enough physics made a turn in the opposite direction at almost exactly the same time.

To Kirchhoff, more especially, it seemed doubtful whether it was justifiable to assign to forces that prominent position to which they were raised by characterizing them as the causes of phenomena. Whether, with Kepler, the form of the orbit of a planet and the velocity at each point is given, or, with Newton, the force at each point, both are really only different methods of describing the facts; and Newton's merit is only the discovery that the description of the motion of the celestial bodies is especially simple if the second differential quotients of their coordinates with respect to the time are given (acceleration, force). In half a page forces were defined away, and physics was made a really descriptive natural science. The structure of mechanics was too firmly fixed for this change in the external aspect to have any essential effect on the inside. The theories of elasticity, which did not involve the conception of molecules, were of older date (Stokes, Lamé, Clebsch). Yet in the development of other branches of physics (electrodynamics, theories of pyro-electricity and of piezo-electricity, and so on) the view gained ground that it could not be the object of theory to penetrate the mechanism of nature, but that this object is, starting merely from the simplest assumptions (that certain magnitudes are linear or other simple functions, and so on), to establish equations as simple as possible which make it possible to calculate the natural phenomena with the closest approximation; as Hertz characteristically says, only to represent nakedly by equations the phenomena directly observed without the variegated garments of hypothesis with which our fancy clothes them.

Several investigators had, before this and from another side, assailed the old system of centers of force and forces at a distance. We might say that this was from the exactly opposite side, because these investigators were particularly fond of the variegated garment of mechanical

representation. It might also be said to be from an adjacent side, as they also dispensed with claims to the knowledge of a mechanism behind the phenomena, and, in the mechanisms which they themselves invented, they did not see those of nature, but mere images or analogies.[2] Several men of science, following the lead of Faraday, had established a totally different view of nature. While the older system had held the centers of force to be the only realities, and the forces themselves to be mathematical conceptions, Faraday saw distinctly the continuous working of the forces from point to point in the intermediate space. The potential, which had hitherto been only a formula for lightening the work of calculation, was for him the bond really existing in space, the cause of the action of force. Faraday's ideas were far less lucid than the earlier hypotheses, defined as they were with mathematical precision, and many a mathematician of the old school had but a low opinion of Faraday's theories, without, however, by the light of his own clear conceptions, making such great discoveries.

But soon, and especially in England, it was attempted to get as visible and tangible a representation of the conceptions and ideas which before had played a part in analysis alone. From this endeavor toward visualization arose the graphical representation of the fundamental conceptions of mechanics in Maxwell's *Matter and Motion,* the geometrical representation of the superposition of two sine motions, and all the visualizations due to the theory of quaternions. Thus, the geometrical interpretation of the symbol

[2] Compare the theory of elasticity worked out by Kirchhoff in his *Lectures,* which is of almost ethereal delicacy, clear as crystal but colorless, with that given by Thomson in the third volume of his *Mathematical and Physical Papers,* a sturdy realistic one, not of an ideal elastic body but of steel, india-rubber, or glue, or with Maxwell's language, often almost childlike in its naivety, who, right in the middle of his formulas, casually gives a really good method of removing grease spots.

$$\Delta = \delta^2/\delta x^2 + \delta^2/\delta y^2 + \delta^2/\delta z^2.*$$

There was another matter. The most surprising and far-reaching analogies were seen to exist between natural phenomena which were apparently quite dissimilar. Nature seemed, in a certain sense, to have built up the most diversified things after exactly the same pattern. As the analyst dryly says, the same differential equations hold for the most diversified phenomena.

Thus the conduction of heat, diffusion, and the propagation of electricity in conductors take place according to the same laws. The same equations may be considered as the solution of a problem in hydrodynamics or in the theory of potential. The theory of vortices in fluids as well as that of the friction of gases exhibits the most surprising analogy with that of electromagnetism, and so on.[3]

Maxwell also, when he undertook the mathematical treatment of Faraday's ideas, was from the very outset impelled by their influence into a new path. Thomson[4] had already pointed out a series of analogies between problems in the theory of elasticity and those of electromagnetism. In his first paper on electricity, Maxwell[5] explained that it was not his intention to propound a theory of electricity; that is, that he himself did not believe in the reality of the incompressible fluid and of the resistances which he there assumed, but that he simply intended to give a mechanical example which shows great analogy with electrical phenomena, and he wished to bring the

* Maxwell, *Treatise on Electricity and Magnetism,* Oxford, 1873, Vol. I, art. 29, "Nature of the operator ∇ and ∇^2. This was also afterwards observed by others: Mach, "Ueber Hrn. Guébhard's Darstellung der Aequipotential-Curven," *Wien. Sitzungsberichte,* Vol. LXXXVI, p. 8, 1882. Compare also *Wied. Beiblätter,* Vol. VII, p. 10; *Comptes Rendus,* Vol. XCV, p. 479.

[3] Cf. on this point Maxwell, *Scientific Papers,* Vol. I, p. 156.

[4] *Cambridge and Dublin Math. Journal,* 1847; *Math. and Phys. Papers,* Vol. I.

[5] Maxwell, "On Faraday's Lines of Force," *Cambridge Phil. Trans.,* Vol. X; *Scientific Papers,* Vol. I, p. 157.

electrical phenomena into a form in which the understanding can readily grasp them.[6]

In his second paper[7] he went still farther, and out of liquid vortices and friction wheels working within cells with elastic sides he constructed a wonderful mechanism which serves as a mechanical model for electromagnetism. This mechanism was, of course, mocked at by those who, like Zöllner, regarded it as a hypothesis in the older sense of the word, and who thought that Maxwell ascribed to it a real existence. This Maxwell decidedly repudiated, and only modestly hoped "that by such mechanical fictions any one who understands the provisional and temporary character of this hypothesis will find himself rather helped than hindered by it in his search after the true interpretation of the phenomena." And they were so helped; for by his model Maxwell arrived at those equations whose peculiar and almost magical power Heinrich Hertz, the person most of all qualified to judge, thus vigorously depicted in his lecture of 1890 on the relations between light and electricity: "We cannot study this wonderful theory without at times feeling as if an independent life and a reason of its own dwelt in these mathematical formulas; as if they were wiser than we were, wiser even than their discoverer; as if they gave out more than had been put into them." I should like to add to these words of Hertz's only this: that Maxwell's formulas were merely consequences of his mechanical models, and Hertz's enthusiastic praise is due, in the chiefest place, not to Maxwell's analysis, but to his acuteness in the discovery of mechanical analogies.

It is only in Maxwell's third important paper[8] and in

[6] Maxwell, *Scientific Papers*, Vol. I, p. 157.

[7] "On Physical Lines of Force," *Phil. Mag.* (4), Vol. XXI, 1861, pp. 161, 281, 338, and Vol. XXIII, 1862, pp. 12, 85; *Scientific Papers*, Vol. I, p. 451.

[8] "A Dynamical Theory of the Electro-magnetic Field," *Phil. Trans.*, Vol. CV, 1865, p. 459; *Scientific Papers*, Vol. I, pp. 526.

his textbook[9] that the formulas more and more detach themselves from the model, and this process was completed by Heaviside, Poynting, Rowland, Hertz and Cohn. Maxwell still used the mechanical analogy, or as he said, the "dynamical illustration." But he no longer pursued it into details, but searched for the most general mechanical assumptions calculated to lead to phenomena which are analogous to those of electromagnetism. Thomson was led, by an extension of the ideas which have already been cited, to the quasi-elastic and quasi-labile ether and to its visualization by the gyrostatic-adynamic model.

Maxwell of course applied the same treatment to other branches of theoretical physics. Maxwell's gas-molecules, which repel each other with a force inversely proportional to the fifth power of their distance, may be conceived as mechanical analogies, and at first investigators were not wanting who, not understanding Maxwell's tendency, affirmed that his hypothesis was improbable and absurd.

The new ideas, however, gradually found entrance into all domains of physics. In the theory of heat I need only mention Helmholtz's celebrated memoirs on the mechanical analogies of the second law of thermodynamics. It was seen, indeed, that they correspond better to the spirit of science than the old hypotheses, and were also more convenient for the investigator himself. For the old hypotheses could only be kept up as long as everything just fitted; but now a few failures of agreement did no harm, for it can be no reproach against a mere analogy if it fits rather loosely in some places. Hence the old theories, such as the elastic theory of light, the theory of gases, the schemes of chemists for the benzol rings, and so on, were soon regarded only as mechanical analogies, and philosophy at last generalized Maxwell's ideas to the doctrine

[9] *Treatise on Electricity and Magnetism*, 2 vols., Oxford, 1873; 2d ed., 1881.

that all knowledge is nothing else than the discovery of analogies. With this the older scientific method was defined away, and science now only spoke in parables.

All these mechanical models at first existed indeed only in thought; they were dynamical illustrations in the imagination and could not be carried out in practice in this general form. Yet their great importance was an incitement practically to realize at least their fundamental types.

In the second part of this catalogue is a description of such an attempt made by Maxwell himself, and of one by the author of these lines. Fitzgerald's model is also at present in the exhibition, as well as Bjerknes's model, which owe their origin to similar tendencies. Other models which have to be classed with these have been constructed by Oliver Lodge, Lord Rayleigh, and others.

They all show how the new tendency to relinquish perfect congruence with nature is compensated by the more striking prominence of points of similarity. To this tendency, without any doubt, belongs the immediate future; yet, mistaken as it was to consider the old method as the only correct one, it would be just as one-sided, after all it has accomplished, to consider it as quite played out, and not to cultivate it along with the new one.

MUNICH, August, 1892. LUDWIG BOLTZMANN.

SUPPLEMENTARY NOTE.

[To the translation in the *Philosophical Magazine* are two additions. One is to note 2, and runs:

"The relation of the directions of the old system of centers of force, and of forces at a distance and the purely mechanical one represented by Kirchhoff, to Maxwell's own point of view is expressed by him in the following words: 'The results of this simplification may take the form of a purely mathematical formula (Kirchhoff), or of a physical hypothesis (Poisson). In the first case we entirely lose sight of the phenomena to be explained and, though we may trace out the consequences of given laws, we can never obtain more extended views of the connections of the subject. If, on the other hand, we adopt a physical hypothesis, we see the phenomena only through a medium, and are liable to that blindness to facts and rashness in assumption

which a partial explanation encourages. We must therefore discover some method of investigation which allows the mind at every step to lay hold of a clear physical conception without being committed to any theory in physical science from which that conception is borrowed, so that it is neither drawn aside by analytical subtleties, nor carried beyond the truth by a favorite hypothesis.' "

The other addition is a reference, in note 3, to B. Riemann's *Electricität und Magnetismus.*

In the Munich exhibition, Boltzmann's mechanical models were: (1) Apparatus for demonstration of the laws of uniformly accelerated rotation;[1] (2) Machine for the demonstration of the superposition of waves;[2] (3) two pieces of apparatus to show the over-tones of plucked strings;[3] and (4) Apparatus for the mechanical illustration of the behavior of two electric currents.[4]

The exhibition also contained, among the mechanical models of electrodynamical phenomena, G. F. Fitzgerald's model to illustrate certain properties of the ether according to Maxwell's theory;[5] Lodge's two models to illustrate certain electrical phenomena;[6] C. A. Bjerknes's model for the hydrodynamical illustration of electrical and magnetic phenomena;[7] M. Möller and O Günther's models for the representation of electrical vibrations and magnetic lines of force about solenoids;[8] Lodge's model for illustration of dielectric displacement according to Maxwell's view;[9] and H. Ebert's apparatus for the mechanical illustration of electrodynamic induction.[10]

Boltzmann's fourth model referred to above was made independently of, but on the same principle as, one already set up by Maxwell in the Cavendish laboratory at Cambridge. Boltzmann's model is described, with another of Lord Rayleigh's which serves the same purpose, in his *Vorlesungen über Maxwell's Theorie der Electricität und des Lichtes.*[11] Boltzmann also published the following papers on mechanical models of physical phenomena: "Ueber die mechanischen Analogien des zweiten Hauptsatzes der Thermodynamik";[12] Ueber ein Medium, dessen mechanische Eigenschaften auf die von Maxwell für den Elektromagnetismus aufgestellten Gleichungen führen; Teil i";[13] "Mechanisches Modell zur Versinnlichung der Lagrange'schen Bewegungsgleichungen";[14] "Ueber die mechanische Analogie des Wärmegleichgewichtes zweier sich berührender Körper";[15] and the article "Models" in the tenth edition of the *Encyclopædia Britannica.*[16]—P. E. B. J.]

[1] *Katalog,* p. 309. [2] *Ibid.,* p. 360. [3] *Ibid.,* pp. 361-362.
[4] *Ibid.,* pp. 405-408. [5] *Ibid.,* pp. 400-401. [6] *Ibid.,* pp. 401-404.
[7] *Ibid.,* pp. 404-405 (with references to the literature on Bjerknes's investigations).
[8] *Ibid.,* pp. 408-410. [9] *Katalog, Nachtrag,* 1893, p. 116.
[10] *Ibid.,* pp. 116-117; cf. Boltzmann's fourth model referred to above.
[11] Leipsic, 1891, sixth lecture.
[12] *Journ. für Math.,* Vol. C, 1886, pp. 201-212.
[13] *Münch. Ber.,* Vol. XXIIa, 1892, pp. 279-301; *Wiedemann's Annalen,* Vol. XLVIII, 1893, pp. 77-99.
[14] *Jahresber. der Deutsch. Math.-Ver.,* Vol. I, 1892, pp. 53-55.
[15] *Wien. Ber.,* Vol. CIII, 1895, pp. 1125-1134.
[16] Vol. XXX, 1902, pp. 788-791.